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## PROOF AND DISPROOF OF CORRELATION.

By C. SPEARMAN.

In the last October number of this *Journal* an article by G. Whipple several times did me the honour of mentioning some work which had appeared there earlier in the year. Unfortunately, however, he does not appear to correctly realized the principles enunciated ; as others may be in the like case and as the principles are believed to be of some importance, perhaps a few words may be allowed here in explanation and example.

For instance, in opposing the correlation alleged by Gilbert<sup>1</sup> between 'brightness' and 'reaction-time,' Whipple asks : 'What evidence have we that the reactions are correlated with mental ability ? When we are told that 'bright' children react more quickly than 'dull,' we find that the quantitative support is afforded by the figures 207, 213 and 224 for the three groups (bright, average and dull, respectively). How much are we within the limits of certainty?';<sup>2</sup> Now, on pages 78-87 of this *Journal*, several methods will be found of calculating the information desired. Or on p. 280 the answer will be found already worked out, showing the amount of correlation in this case to be  $0.19 \pm 0.94$ ; this means that the influence of the common factor as compared with those not common is as  $0.19^2$  to  $1 - 0.19^2$ , or as 3.6 to 96.4 (see p. 273); moreover, the correlation being nearly five times as large as the probable error, this correlation, though so small, has a certainty of about 1,000 to 1 (see p. 76).

"Again, Whipple argues that reaction-times cannot really be correlated with 'brightness' to any appreciable extent, because under strict conditions of test the reaction-times of all persons of the same type (that is, sensorial or muscular) are very nearly equal. Waiving the question of fact, even the principle cannot be admitted; correlation does not depend upon the greatness but upon the constant direction of deviations. For instance, the sharpest eyes cannot detect the tiny changes daily taking place in a bottle of mercury; yet these changes, though so minute, are all the time in almost perfect correlation with the temperature—and may be made to serve as a very delicate measure of it, on pouring the mercury into a thin, graduated tube. Had Gilbert's reaction-times been taken under very strict conditions of test, the correlation, instead of disap-

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<sup>1</sup> Studies from Yale Psych. Lab., Vol. II.

<sup>2</sup> Page 495.

pearing, as Whipple concludes, might have become much higher."

Further, he quotes the work of Wissler<sup>1</sup> as conclusively demonstrating that "the marking of A's, the naming of colors, the execution of rapid movements and the recording of simple association fail to show any significant correlation with one another."<sup>2</sup> Whipple recognizes that the tests in question were much too few and hurried to give at all truly representative results, but he considers this defect to be made good by the exhaustiveness and other merits of these experimental series; as the correlation turned out to be about nil time after time without exception, the general negative results may, he thinks, be accepted with confidence. This is, again, fundamentally erroneous; a series of irregular results, however excellent otherwise, can never disprove correlation; there is nothing to show that correlation does not really exist and has only been attenuated away to nothing by the irregularities; and if this occurs in one experimental series, it will punctually reoccur in every other series where the results are equally irregular (see pp. 89, 91 and 92).

On the other hand, he treats alleged "roughness" in some experiments of my own much more severely, as he argues that it vitiates the obtained correlations.<sup>3</sup> But I must point out that, though roughness or irregularity may perfectly well be cited to invalidate negative results as in Wissler's case, it can never be urged against positive ones as in my case; mere irregularity always *reduces* correlations, never conjures up illusive ones. Positive correlations can only be shaken by showing that the probable error is not much less than the correlation itself or that both terms are strongly influenced by some irrelevant *constant* factor; thus, the correlation found between intelligence and sensory acuteness would really be explained away if it could be shown that the variations in both were merely due to differences of intellectual maturity. Instead of the alleged roughness disproving the truth of the correlations, the highness of the correlations conclusively disproves the alleged roughness. Fortunately, Whipple has carefully mentioned wherein this roughness consists; he infers that the following points have been disregarded: moderate and equal intensity of sound, preliminary 'warming up' and favorable time-interval between the compared stimuli.<sup>4</sup> But how he came to conceive that these points were disregarded I cannot conceive; they would scarcely be neglected by a junior student; I am afraid that he must have omitted to peruse pages 243-47, where

<sup>1</sup> Psych. Rev. Monograph Supplement, June, 1901.

<sup>2</sup> Page 496.

<sup>3</sup> Page 496-97.

<sup>4</sup> Page 497.

account is given of the special precautions taken to secure all these desirable conditions. He also considers it improbable that proper attention was paid to the influence of practice; he would appear to have overlooked the six full pages devoted solely to describing the quite unusually elaborate researches and precautions taking to obviate disturbance from this cause. Most of all he emphasizes, as a disturbing factor discovered by himself, the fact that the power of discrimination may in exceptional cases vary enormously with the kind of timbre; but this fact had already been announced and discussed in my own work.<sup>1</sup> As far as I can see, the real grounds of his want of confidence are that the reagents were children and the tests did not take place in a laboratory. He will, therefore, be glad to hear that the experiments have since been repeated upon adult students of psychology and in an irreproachable laboratory; that upon this occasion I had the advantage of a co-worker so deservedly well known in acoustical psychology as Dr. F. Krueger; and that our results qualitative and quantitative were precisely similar to those previously published.

Before concluding, I should like to emphatically disclaim ever having recommended the test of pitch-discrimination (or anything else!) "as a universal and ready means for the estimation of General Intelligence" of any person; all I did was to point out that mathematical examination papers had shown themselves scarcely more reliable. I must also protest that he estimates too low the general deductions from the correlations when he sums these up in the harmless identity that "General Intelligence is General Intelligence." The correlations show that something which may be called General Intelligence *exists*; that all Specific Intelligences (within the range of these experiments) deal with intellectual ranges of vanishingly small dimensions; that the relative influences of the General and the Specific Intelligences respectively may be measured with great accuracy<sup>2</sup>. These facts are a germ which may eventually develop into immense practical reforms and, possibly, a theoretical revolution. On the other hand, Whipple appears to me to estimate too highly the current information concerning the real nature of this General Intelligence; he treats it as a simple matter requiring no further consideration. To me, at any rate, it is by no means so conveniently obvious, and Whipple's popular explanation appears, I am afraid, lacking in depth;

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<sup>1</sup> Page 239.

<sup>2</sup> This is no estimate of the Intelligence of any person but only the first step in that direction—an estimate of any proposed *test* of Intelligence.

far from starting with a clear definition of General Intelligence, I had to content myself with using the term as denoting an almost unknown X "implying nothing more than a bare unequivocal indication of the factual conditions of the experiment."<sup>1</sup> To solve this X I proposed, and have since been gradually carrying out, a long investigation upon strictly experimental lines; the results up to now do not corroborate the popular assumptions.

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<sup>1</sup> Page 250.